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ORIGINAL ARTICLES.

DIABETES.

A. L. BENEDICT, A. M., M. D.,* BUFFALO, N. Y.

In these pragmatic times, when theorists are scoffed at, and when even the scientific investigator describes his experiments and their results, but stops short of the conclusions and explanations that force themselves upon his attention, one may well apologize for an article that is avowedly theoretical and speculative. Yet, true knowledge is something more than an acquaintance with facts; it includes the understanding of relations and effects. It is a question whether, in the last two decades, we have not paid too much attention to the anatomy of science and too little to the physiology. In the practical study and management of diabetes there is needed not so much a list of drugs and doses as an insight into the nature of the disease which shall render us competent to choose intelligently from among the many suggestions. We need, not so much a wide familiarity with symptoms and complications, as the power to discriminate between significant phenomena and those which are merely accidental.

What is diabetes? It is literally a

"passing through," and if we pause here, our conception will be more correct than if we proceed to qualify our definition by reference to the presence of sugar in the urine. Without any scientific support, the idea has prevailed—and it still rises to the surface with undiminished pertinacity—that diabetes is a disease of the kidney whose essence is an elimination of sugar. Now, the normal kidney removes from the blood whatever is abnormally present, whether it be foreign matter or an excess of normal constituent. Egg albumin, peptone, coloring matter, introduced into the blood, are eliminated by the kidney, but the resulting abnormality of the urine is no indication of renal disease. One might as sensibly argue that the presence in a station-house of an unusual number of criminals was evidence of the improper discipline of the police force. It is almost incomprehensible that a knowledge of medicine has occasioned such a loss of common sense that men have, blamed the kidneys for arresting dangerous ingredients of the blood and passing them on to the bladder. It is true that the irritation of a long-continued elimination of sugar

*Lecturer on Diseases of the Digestive Organs, Dental Department, University of Buffalo.

may induce parenchymatous degeneration of the kidneys, but this nephritis has no more relation to the primary disease than has pruritus of the external genitals, due likewise to the irritating quality of the urine.

In diabetes, there is not only a waste of sugar, but an excessive elimination of urea and the latter is usually relatively and often absolutely greater than the former. Acetone, diacetic acid and other positively toxic substances may also find their way into the urine after doing damage in the blood, but these are rather the result of a severe diabetes of long standing than part of its usual chemism, and the main element of harm in diabetes is the extraordinary waste of nitrogenous matter, typified by the excessive elimination of urea.

In tracing diabetes back from the kidney to the blood, we have made but one step in our study of its nature. Glucose occurs normally in the blood in the proportion of 9:10,000. In diabetes, the ratio is increased to about 3:1,000. The question immediately presents itself, why is there this hyperglycæmia? Is it because too much sugar enters the blood, or because too little leaves it by other routes than the kidney?

Before attempting to answer these questions, we must review the normal entrance into and exit from the blood, of sugar. Starchy foods, if cooked, thoroughly masticated and present in not too great quantity, are converted into maltose by the saliva, in the stomach, during the first hour of digestion. If these conditions are not fulfilled, or if there is superacidity of the stomach, one of the pancreatic ferments accomplishes the same change. Milk sugar and cane sugar, which are scarcely acted upon by these ferments, have the same empirical formula as maltose, $C_{12}H_{22}O_{11}$, and all of this group are changed by the intestinal ferment into the forms of glucose, dextrose and levulose, distinguished by their action on polarized light. Diabetes has even been ascribed to a failure of carbohydrate digestion, though it ought to be obvious that, if too little sugar is formed, there can scarcely be too much in the blood.

Approximately speaking, all the nutriment of the body, except fat, goes to the liver through branches of the portal

vein and is there elaborated before reaching the general circulation. At least in the case of sugar, the liver has the function of the tender of an engine, being loaded three times a day with fuel which is continually passed on to the body, as needed. The blood of the entire body amounts to between 5 and 6 kilograms and 9:10,000 of glucose, or between 4.5 and 5.4 grams are constantly circulating. It is a simple arithmetical problem to show that, after the average meal, the liver has received about thirty times as much sugar as should circulate at any one time, and that this must be held back and supplied in infinitesimal amounts to the blood of the hepatic vein. This is accomplished by changing the glucose into glycogen, a substance having the same empirical formula as ordinary starch, $C_6H_{10}O_5$, but differing in many important chemical and physical properties. It should be stated that other glands, muscles and even leucocytes in the inflammatory foci or throughout the system, have the same power of fixing glucose as glycogen, just as respiration, though the special function of the lungs is carried on to a minor degree by the skin, intestine, and probably, all active cells of the body.

In diabetes, there is apparently a failure of the glycogenic function of the body, yet, strangely enough, serious disease of the liver is not usually followed by diabetes, nor, conversely, does liver disease occur with any such frequency in diabetics as to warrant us in considering it an essential part of the morbid anatomy of disease. In short, any trouble with the liver which we may consider as characteristic of diabetes, must be admitted to be purely functional. This, however, does not preclude the possibility of its being due to an organic nerve lesion, as for instance, injury of the floor of the fourth ventricle, or degeneration of the celiac plexus.

Under normal circumstances, glucose is the main food or fuel of the body. Nitrogenous foods are never completely oxidized, and serve chiefly to replace broken-down tissue. It is true that life may be supported by nitrogenous foods alone, but not economically, and only because such foods are first decomposed

into nitrogenous waste, of which we may consider urea the type, though not the accurate representative, and into fat, carbohydrate or some similar fuel. Fat is capable of complete oxidation in the body, and hence is theoretically the best food for furnishing heat and power, but it is relatively indigestible, and it is perfectly obvious that the ninety grams which constitutes the average daily ration is insufficient for the purpose. Carbohydrates are demanded to the extent of 450 or 500 grams daily, and are the main source of heat and power. They are utilized as glucose in the blood, and they may even be changed into fat and deposited as such.

A glance at the composition of the molecule of glucose, $C_6H_{12}O_6$, shows us that, whatever may be the exact process of oxidation, there is already sufficient oxygen for the complete oxidation of all the hydrogen, so that the fuel value as expressed by the carbon and the oxidation product must be CO_2 . We come now to some facts that are of the utmost significance in enabling us to form a correct conception of diabetes. Sugar absorbed into the blood of a healthy person is practically completely oxidized, either in the liver or other glands, muscles, or the blood and tissues generally, and is excreted as H_2O and CO_2 . An increased ingestion of carbohydrates is followed by a corresponding increase of CO_2 eliminated—it is manifestly of great practical difficulty to distinguish between water formed in the body and that which simply passes through—and an enormous amount of sugar must be ingested, 100 grams or more at one feeding, in order to produce even a temporary glycosuria. It is practically impossible to cause glycosuria in a healthy person by carbohydrates, unless already in the form of sugar. The diabetic, on the other hand, does not excrete more CO_2 if his saccharine or starchy food is increased. He wastes away in spite of—rather on account of—a liberal diet containing the natural fuel of the body. We are now prepared to say that the hyperglycemia of diabetes is due to decreased oxidation, not to increased formation or ingestion of sugar. A little consideration will convince us that the comparison of glucose to fuel is rather a figure of

speech than a literal truth. Such fuel will not burn at the temperature of the body, nor will it oxidize in any sense unless under the influence of some ferment. Living ferments, such as bacteria and yeasts, are out of the question and we must assume, *a priori*, that healthy blood contains an unorganized ferment, capable of inducing the oxidation of glucose at the temperature of the body. The presence of such a ferment has been demonstrated as fully as may be without isolation. This ferment has also been shown to be deficient in the blood of diabetics, by comparing the latter with the blood of healthy persons. It is doubtful if the glycogenic function of the liver, in the crude sense in which the term is usually applied, is really at fault in diabetics. There are no experiments showing that glycogen is absent from the livers of diabetics, nor that the proportion of sugar in the blood rises suddenly after a carbohydrate meal, as would be the case if the liver were absolutely unable to retain glycogen. Hyperglycemia, on the other hand, is quite uniform in the same diabetic at different times of the day, and varies only gradually under the influence of diet.

The connection of the pancreas with diabetes was established clinically long before any plausible explanation was offered. Setting aside the difficulty of reconciling an excess of sugar in the blood with the failure of a merely digestive organ, it was proven experimentally that diabetes did not occur if the pancreas was simply severed from its connection with the intestine, nor even if the greater part of the organ were extirpated, leaving a small mass entirely disconnected from its duct. In some experiments it even occurred that, after the fragment of pancreas had been left in the body for a time, either in its normal location or as a graft in some unusual location, even this vestige might be subsequently removed without producing diabetes. All these apparently contradictory results could only be reconciled, as Lepine pointed out, by assigning to the pancreas not only its digestive function, but that of a ductless gland, producing an internal secretion which some other organ might manufacture vicariously under proper circumstances. Further experiments have

shown that the blood of the pancreatic vein is rich in glycolytic power, and that this power is not present, or present to a slight degree, in blood from the same vein of a diabetic animal.

By careful elimination of carbohydrates from the diet, we may, within a few days, reduce the amount of sugar circulating in the blood and removed in the urine, to a considerable extent. Thus a week's dieting in a case now under treatment, reduced the sugar from 300 to 75 grams. in the twenty-four hour urine. It is also well established clinically, that the addition of a small amount of carbohydrate nourishment will cause the excretion of a far greater amount of sugar in the urine than can possibly be accounted for in the food. Why? Perhaps because the presence of a small amount of sugar stimulates the formation of more in the system; perhaps the small amount of glycolytic ferment which we may imagine to be retained by the diabetic is exhausted by the added sugar. The latter theory is scarcely tenable. Under carefully restricted diet the sugar sometimes disappears, but usually reaches and remains at a figure which maintains a uniform ratio to the elimination of urea. We must then conclude that the sugar is formed from albuminoids, either ingested or existing in the tissues.

So far as the patient is concerned, the essence of diabetes is the wasting of the body. We must not be misled by our chemical study of a case into imagining that the mere excess of glucose in the blood or urine is harmful, except as it acts as an irritant. Nor is the breaking up of albuminous food into sugar and urea—or some similar substance—more than a wasteful state of assimilation, such as we can duplicate without special harm, by an artificial limitation to meaty diet. The true significance of the excess of sugar is that the body lacks the ferment necessary to the utilization of the proper fuel—glucose. If the body could fall back on a nitrogenous and fatty regimen, comparatively little harm would result, and we do occasionally find just such cases of diabetes in which the disease is held in check, sugar caused to disappear from the urine and the patient restored to a state of comparative health by the exclusion

of carbohydrates from the food. Fat however, is not a good physiological fuel. I have under observation a patient who has lost weight and strength and has practically emaciated under a thick padding of subcutaneous fat. Doubtless, actual starvation would cause the utilization of this fat, but it is evidently not available as fuel so long as the system has any choice. Albumin may be separated into nitrogenous waste on the one hand and fat or sugar, or some similar substances, on the other. The former dissociation occurs in fatty metamorphosis, the latter in those intractable cases of diabetes in which sugar persists in the blood, after the most rigid dieting. I would like to advance the hypothesis that the cases in which dieting effects a provisional cure, without serious sacrifice of nutrition, are those in which the chemistry of the dissociation of the albuminoid molecule is such that an oxidizable substitute for sugar is formed.

We may consider, then, that the pathology of diabetes, according to our present light, is somewhat as follows: Either from organic disease or from defect of innervation, due to lesion of the medulla, degeneration of the coeliac plexus or some purely functional disturbance of the sympathetic, the pancreas fails to secrete the necessary glycolytic ferment. Glucose thus accumulates in the blood and is excreted in the urine, since it cannot be oxidized into CO_2 and H_2O . The body, unable to utilize its normal fuel, decomposes albumin, with resulting enormous increase of nitrogenous waste and, at least in some instances, the production of more sugar. Upon the ability of the body (1) to use fats, (2) to break down albumin into something not quite the same as sugar, as well as nitrogenous waste, (3) to use the albumin of food rather than of the tissues, depends the favorable course of the disease in some persons. On the other hand, a chemism in which diacetic acid, acetone, etc., are formed, is most disastrous. The glyco-genic function of the liver, that is, the power to fix glucose as glycogen and reconvert the latter into glucose gradually, does not seem to be particularly involved. In short, the liver is rarely at fault and treatment directed toward

it is illogical. If, however, it can be shown that, in any particular case, the excess of sugar begins in the hepatic vein, that the liver after carbohydrate feeding is destitute of glycogen, or that sugar appears in the blood *pari passu* with its absorption from the stomach or intestine, the trouble may rightly be ascribed to the glycogenic function of the liver. The waste of nitrogenous substances must be considered as an effort on the part of the body to introduce into the blood an oxidizable non-nitrogenous substitute for glucose. If this substance is sugar and the glycolytic ferment is absolutely wanting, there will still be an excessive elimination of saccharine urine. If the substance formed is oxidizable without the glycolytic ferment, or if there is a little of the latter formed, the progress of the case under restricted diet will be favorable. The factors that determine whether the system can utilize fats or whether even albumins are not assimilated so that the body must prey on its own tissues, are veiled in mystery, but no deeper than similar problems in persons in ordinary health. In some cases, notably when acetone, diacetic acid, etc., are formed, the administration of carbohydrates seems to be rather beneficial than otherwise.

Diabetes may be considered to be a functional disease of the pancreas. That is, though serious diseases of the pancreas are usually attended by diabetes, many cases of diabetes are not marked by pancreatic lesion. Thus, Williamson found the pancreas to be absolutely normal, both macroscopically and microscopically, in seven out of fifteen cases of diabetes, while the remaining eight cases were attended by so diverse lesions that we can regard no one of them as characteristic. It is not, however, possible to supply the internal secretion of the pancreas by feeding with fresh pancreas or its extract, though it may prove that hypodermatic treatment will afford better results. If we have to deal with serious organic disease of the pancreas, we can not hope to produce a permanent cure, though we may so modify glycolysis and nitrogenous waste as to make the claim of the Irish physician that his patient "died cured." If the condition is functional so far as the pancreas is

concerned, but due to serious disintegration of the coeliac plexus, lesion of the floor of the fourth ventricle, or of other dominating sympathetic centers, anything more than temporary relief of some of the symptoms is out of the question. If the condition is functional, both with regard to the pancreas and the sympathetic system, we may hope to find some remedy which will restore the nervous tone and effect a cure. The more strongly the history of the case points to a sudden nervous impression as the cause, the more may we hope for a permanent cure, except in those fulminating cases occurring in youths, in which the important question is not between functional and organic nature, but in which death ensues from lack of vitality and lack of time to combat nutritional symptoms. It is probable that so-called diabetes occurring in elderly persons as the result of high living, is more properly a glycosuria, since the disease is quite amenable to treatment and is not attended with malnutrition of serious grade. Even the increase of nitrogenous waste to a moderate degree, may be explained as due to superalimentation.

While it is true clinically that most urines which reduce an alkaline copper solution belong to diabetics, we must bear in mind, (1) that an excess of uric acid, kreatinin, salicylic acid, and the more or less altered remains of drugs such as morphine, chloral, nitrites, copaiba, etc., will give the same test. It is a matter of dispute whether the substance which reduces copper after the administration is really sugar or not. Sajou's annual of one year apparently settles the matter in the negative, but the next edition again refers to the excreted substance as sugar. (2) We must remember that if glucose is actually present in the urine, and in excess in the blood, it may be due to a temporary condition. It is in such cases rather than in true diabetes that the glycogenic function is at fault or, perhaps, has simply been taxed beyond reason, by dietetic excesses. (3) Injuries to the brain and skull, spinal cord and other important nervous centers, systemic or sympathetic; brain and cord diseases not usually marked by diabetes or even glycosuria; pregnancy; grief, hysteria and other transient strains on the ner-

vous system; all predispose to glycosuria, so that the ingestion of a relatively slight excess of carbohydrate nourishment may be followed by sugar in the urine.

One occasionally encounters—with or without demonstrable hepatic disease—an enormous increase of urea in the urine, without sugar. We may well ask if these cases are not essentially diabetic, but with the persistence of the glycolytic ferment in sufficient amount to cause the oxidation of all the sugar formed from

albumin. Whatever may be the correct answer to such speculations, we must change the old conception that diabetes is a saccharine disease, and substitute the conception that the main element is the waste of nitrogenous food or tissues while the hyperglycemia and glycosuria are secondary and irritating rather than essential conditions. Hence, in the examination of the urine, it is the urea rather than the glucose that we must estimate more carefully and to which we must attach the greater importance.

IS ALCOHOL A STIMULANT?

J. M. FRENCH, M.D., MILFORD, MASS.

Webster defines a stimulant, medically considered, as an agent which produces an increase of vital action in the organism, or any of its parts. In discussing the question whether or not alcohol is properly entitled to be classed as a stimulant, let it be understood that I am considering the primal basic action of the drug, and not any special or incidental effect. The question is not whether alcohol is capable, in particular organs and under certain conditions, of producing results which either are, or at first sight closely resemble, the effects of stimulation; but whether stimulation is its fundamental, universal, and most important effect. To illustrate, chloroform is capable, under certain conditions, of producing vesication; but no one would think of classifying chloroform as a counter-irritant. I have known quinine, several times in the same individual, to produce a condition so closely resembling scarlatina as to be unhesitatingly pronounced as such by a consulting physician of wide experience. [See *MEDICAL AND SURGICAL REPORTER*, September 8, 1888]; yet I do not look upon quinine as a specific disease germ.

I am aware that the theory of the stimulant action of alcohol is held by a large proportion—probably a large majority—of the medical profession, and that it has the sanction of a long line of

authority. Since its dawn in history, alcohol has almost universally been regarded as the type and acme of stimulants. It has been used to promote health and prevent disease; to increase appetite and digestion, and create strength; to relieve pain, revive the spirits, and avert the blues; to overcome depression, prevent heart failure, induce reaction in shock, and bridge over the period of crises in fever; and always and everywhere on the theory that its fundamental action is that of a stimulant.

My contention is, on the other hand, that the basic action of alcohol is never that of a stimulant, a tonic, an excitant of vital activity; but always the exact opposite—that of a sedative, an anæsthetic, a paralyzant; that whenever any apparent stimulant effect results from its use, such effect is always incidental, partial, and at the expense of other and more important contrary actions; that for purposes of stimulation we possess other agents, simpler, more positive and more reliable in their action, as well as more economical of vital force; and hence, that the occasions when alcohol may properly be used for this object are very few.

In looking over his armamentarium, the physician finds three important agents which are closely related. These are alcohol, ether, and chloroform.

Chemically considered, they resemble each other in their origin and nature. Ether is formed from alcohol by the action of sulphuric acid, and chloroform by the action of chlorine gas. The formula of alcohol is made up by uniting the radical C_2H_5 with the radical $H O$. That of ether is derived from this by substituting a second radical C_2H_5 , in place of the atom of hydrogen HO thus— C_2H_5 , $C_2H_5 O$. Common alcohol is ethyl alcohol; ether is ethyl oxide.

Therapeutically, also, they resemble each other very closely. Who that has ever witnessed the process of etherization, or the production of anæsthesia by chloroform, has failed to note the close correspondence to the steps of alcoholic intoxication? And wherein lies the difference between a man thoroughly under the influence of an anæsthetic on a surgeon's table, and one who lies dead drunk in the gutter? In the slang of the street it is said of a drunken man, "He is paralyzed"; and how can we more perfectly describe the subject of anæsthesia?

There is another group of remedies which the physician carries in his pocket case for emergencies, not less indispensable than those I have mentioned, but very unlike them in properties. These are atropia, strychnia, and digitalis—remedies unexcelled, unequalled, unapproached, for their proper uses, by any in the *materia medica*—potent for good and equally so for evil.

These two groups of remedies are physiologically antagonistic, therapeutically opposed to each other. The one are paralyzers; the other are stimulants. The one diminish and prevent vital action; the other increase and strengthen it. And alcohol is a paralyzant, not a stimulant.

Dr. H. C. Wood, of Philadelphia, in an address on anæsthesia, before the Tenth International Medical Congress, at Berlin, in 1890, stated in substance that nothing could more conclusively demonstrate the power of alcohol as a paralyzer of both the heart and respiration than the experiments which were detailed by him in that address. His conclusions were, that both clinical facts without number, and direct physiological experiments executed by himself and numerous other scientific observers,

show that each of these three drugs, alcohol, chloroform, and ether, taken separately or all in combination, act as direct paralyzers, first of the cerebral hemispheres, producing anæsthesia, and next of the respiratory and cardiac ganglia, thereby inducing death either by failure of the respiration or circulation, or by both simultaneously. These three agents act in direct proportion to the quantity used, there being no dose, however small, and no stage in the progress of their influence, when they increase either respiratory or cardiac efficiency. Acting in the same direction and on the same important functions, each intensifies and increases the effect of the others. In the closing paragraph of the address, in speaking of the management of accidents during anæsthesia, he uses these words: "Avoid the use of all drugs except strychnine, digitalis, and ammonia. Use artificial forced respiration promptly, and in protracted cases employ artificial warmth and stimulation of the surface by the dry electric brush, etc.; and remember that some, perhaps many, of the deaths which have been set down as due to chloroform and ether, have been produced by the alcohol which has been given for the relief of the patient."

Dr. A. S. Davis, of Chicago, a physician of wide reputation and experience, a professor in the Chicago Medical College, one of the founders and twice president of the American Medical Association, who has given life-long study to the action of alcohol, says: "It has long been one of the noted paradoxes of human action, that the same individual should resort to the use of the same alcoholic drink to warm him in winter, to protect him from the heat of summer, to strengthen him when weak or weary, and to soothe or cheer him when afflicted in body or mind. All this is easily explained. The alcoholic drink does not relieve the individual from cold by increasing his temperature, nor from heat by cooling him, nor from weakness and exhaustion by nourishing his tissues, nor yet from affliction by increasing his nerve force; but simply by diminishing the sensibility of the brain and nerves, and thereby lessening his consciousness of impressions of all kinds, whether of heat or cold, weariness or

pain. The same anæsthetic properties that render the laboring man less conscious of the cold or heat or weariness, also renders the sick man less conscious of suffering, either mental or physical, and thereby deceives both him and his physician by the appearance temporarily of more comfort. It is true that a physician can make the anæsthetic properties of alcohol available for the temporary relief of pain and the production of sleep, but it is equally true that he has many other remedies more efficient for the purpose and less objectionable than alcohol."

Somewhat in line with this testimony of Dr. Davis, are the following words of Dr. Edward Long Fox, President of the British Medical Association, in his annual address before the sixty-second annual meeting of the association: "How important it is for a nation to know that any excess in muscular work, as in the forced march of an army, is rendered far more difficult by the use of alcohol; that it not only fails in giving power in the work of the muscles and the heart, but acts distinctly as a depressant; that it never enhances the temperature of the body; and that in its pure state it is in no sense a food. All this knowledge has been gained by the observation of medical men."

I shall next call your attention to a series of experimental inquiries concerning the physiological action of alcohol, undertaken some three or four years ago, by Dr. J. H. Kellogg, of Battle Creek, Mich., editor of *Modern Medicine* and proprietor of the largest sanitarium in America. His researches relate to five lines of inquiry, as follows: 1. The influence of alcohol upon nerve sensibility, relating especially to the tactile sense and the temperature sense. 2. The influence of alcohol upon the rate of mental action. 3. The influence of alcohol in small doses upon muscular coördination. 4. The influence of alcohol upon muscular strength. 5. The effects of alcohol upon digestion.

These experiments were carried out by means of instruments and methods of precision, and were elaborate and exhaustive. They are described at length in a paper read by the author in May, 1893, but are far too lengthy to be given here. Suffice it to say, that after de-

scribing his experiments in detail, he gives his conclusions as follows: "From the facts above given," he says, "it may fairly be concluded that the results of the administration of one ounce of alcohol internally are as follows:

- "1. To diminish nerve activity.
- "2. To diminish cerebral activity.
- "3. To impair the coördinating power of the brain.
- "4. To lessen muscular strength.
- "5. To decrease digestive activity to a notable extent."

It will be observed that none of these effects are those of increase of vital action, but all of its diminution. It would seem that sufficient evidence had been adduced to satisfy the most skeptical "that alcohol is a narcotic and an anæsthetic rather than a stimulant; and that its use as a tonic and supporting remedy is a practice without foundation in either scientific theory or clinical experience. Nevertheless, by the multitude of testimonies the facts may be more firmly established, and I will therefore quote again from Dr. Davis, as follows: "The accepted definition of a stimulant in medical literature is some agent capable of exciting or increasing vital activity as a whole, or the natural activity of some one structure or organ. For instance, both clinical and experimental observations show that strychnine directly increases the functional activity of the respiratory, cardiac and vaso-motor nervous systems, and thereby increases the internal distribution of oxygen, which is nature's own special inciter of vital action. Therefore it is properly a direct respiratory, cardiac and vaso-motor stimulant, and indirectly a stimulant of all vital processes. But the same kind of chemical and experimental observations show that alcohol directly diminishes the functional activity of all nerve structures, pre-eminently those of respiration and circulation, and also all metabolic processes, whether respiratory, disintegrative, or secretory. Consequently it not only acts directly antagonistic to strychnine, but equally so to all true stimulants or remedies capable of increasing vital activity."

Lastly, I will call your attention to the testimony of Dr. Benjamin Ward Richardson, of England, on the so-called

stimulant action of alcohol on the heart. "Recently," he says, "physiological research has served to explain the reason why, under alcohol, the heart at first beats so quickly; why the pulses rise, and why the minute blood-vessels become so strongly injected. At one time it was imagined that alcohol acts immediately upon the heart by stimulating it to increased motion; and from this idea of the primary action of alcohol, many erroneous conclusions have been drawn. We have now learned that there exist many chemical bodies which act in the same manner as alcohol, and that their effect is not to stimulate the heart, but to weaken the contracted force of the extreme and minute vessels which the heart fills with blood at each of its strokes. These bodies

produce, in fact, a paralysis of the organic nervous supply of the vessels which constitute the minute vascular structures. The minute vessels when paralyzed offer inefficient resistance to the force of the heart and the pulsating organ thus liberated, like the mainspring of a clock from which the resistance has been removed, quickens its action, dilating the feebly-resistant vessels, and giving evidence really, not of increased, but of wasted power."

And so even this stronghold of the advocates of the stimulant action of alcohol is broken down, and the increased action of the heart and flushing of the surface which result from taking alcohol, are shown to be the result, not of stimulation of the heart, but of paralysis of the vaso-motor nerves.

THE TREATMENT OF CROUPOUS PNEUMONIA.

ROBERT C. KENNER, A.M., M.D., LOUISVILLE, KY.

Quain says: "Our object must be to endeavor to conduct the pneumonia to a favorable termination. We cannot arrest its progress, but we can often do very much both to maintain the strength of the patient and to modify those elements in the disease which tend to destroy life." This truth should deter us from any attempt to abort the disease by any means calculated to depress the system, and in that way diminish the power of resistance on the part of the patient.

It is important that the sick-room should be light and properly ventilated. Juergensen says: "In my opinion, patients who are exposed to the light make the best recovery." Evidence of the prejudicial effect of dark rooms is abundant in the writings of good observers. The light should not fall into the face of the patient, but the bed should be placed in such a position that, without annoying him he can obtain its cheering and tonic influence. The temperature of the room should not be allowed to fall below 60° F., nor rise above 65°. A thermometer should be hung in the room and strict attention

given to this matter. When the temperature rises above the desired point, the top of one or two windows can be let down, and the desired mean in this way may be secured. I find it often serviceable to leave the top of a window permanently down; but the same purpose can be frequently secured by leaving open a door that does not communicate directly with the external air. The bed-clothes should be suited to supply comfort to the patient. It will be difficult often to get children to lie in bed, and they will cry to stay in the nurse's or mother's arms. In such cases it is important to caution the nurse against allowing the feet to be exposed. Attention to this and other details is of the greatest importance, since failure to observe them is frequently followed by severity in all the symptoms, and often relapses when convalescence is thought to be fairly established. The chest should be incased in cotton batting, and this covered with oil-silk or flannel. This affords a needed protection from draughts, promotes diaphoresis, and tends greatly to the comfort of the pa-

tient. I have these put on in such a way that I can have counter-irritants applied without a great deal of trouble. Ammonia liniment every three or four hours often contributes to the relief of pain, and the same may be said of aconite liniment, and many medicaments of that class. Poultices are often harmful. Especially is this true if we have other than a very attentive nurse, who would allow them to become cold. Blisters are recommended by Loonis to be used in the third stage in the hope of expediting the removal of the exudation. Yet I never, however, use them in this stage, because I believe their employment affords us no good results, but often gives the patient great annoyance. But their use in the first stage is often attended with the most marked benefit in relieving the pleuritic pain so often present. I have found them valuable, and use them in all cases where the pain is a feature that racks the patient considerably. Of course, where the blisters are used the batting jackets are not to be employed.

Much has been written sometime ago advocating a return to the practice of Wood and Watson, and the older writers, of bleeding in pneumonia. Harts-horne, of Philadelphia, has advocated this means, and has brought the statistics of several hospitals to show that when bleeding was an essential element of the treatment, more patients recovered than under the present treatment. These statistics are not valuable for many reasons: (a) They are not large. (b) They do not attempt to define the physical conditions of the patients at the time of entrance, (c) or state the amount of pulmonary involvement. Without going to other statistics for proof of the fallacy of Hartshorne's statements, these points are sufficient to make his conclusions less weighty than they seem.

Bleeding is often useful to postpone asphyxia, and this is an indication for its use in pneumonia. Flint says that it is allowable in plethoric persons, but I think that there is a tendency on the part of this disease to depress the vital forces to such an extent that it is never advisable, unless as a means, as before stated, to avert impending death from asphyxia. Wilson Fox sums up the

present knowledge of bleeding in the following manner: "(1) That indiscriminate bleeding immensely increases the mortality of the disease. (2) That it is specially fatal in old people and young children, in patients of exhausted constitution, and in those suffering from chronic diseases, and particularly from Bright's disease. (3) That it is absolutely unnecessary in the majority of cases of young adults and also young children. (4) That in the majority of cases it has no influence whatever, either in cutting short the disease, in lessening its duration, or in diminishing the pyrexia; but that occasionally these results appear to follow from its use when practiced early. (5) That in the majority of cases it hinders the critical fall of temperature and delays convalescence. (6) That in the majority of cases, as shown, especially by Bennett's and Didel's data, recovery is equally, if not more rapid when it is not practiced as when it is resorted to. (7) That in a few cases a moderate venesection may be necessary in the early stages to avert imminent danger of death from asphyxia."

For the first four days there is generally more or less pain. This, of course, varies in different cases and different individuals. In some it is very severe and agonizing, while in others it amounts to little more than an uneasiness. The use of opium is necessary during this time in such amounts as will secure freedom from pain and unrest. Sometimes the pain is so intense as to require the frequent administration of morphine. Morphine given in this way goes a long way toward preserving the strength of the patient, and of ultimately preventing "failure of cardiac power," which, as Quain truly says, "is the great source of danger." When the pneumonic infiltration is complete, which is usually about the fourth day, the pain is no longer a symptom, and further use of opium is not called for, as, of course, its use would be attended with positive harm should it be continued. It might tend to paralyze the bronchial tubes and favor an accumulation of mucus in them.

One of the most important indications in the treatment of this disease is the proper use of stimulants. I regard nearly all cases which come under the

observation of physicians as requiring the administration of stimulants. Their use is called for in such quantities and at such intervals as will give their weak pulse better volume. It is my practice, unless the patient impresses me as being positively plethoric, to begin the use of stimulants at my first visit, and have them continued. I am satisfied that by this means I have prevented many cases of cardiac failure. The amount and frequency of the administration will depend always on the urgency of the cardiac symptoms. In this connection Juergensen well says: "To lay down a set of rules for the administration of stimulants would be a very thankless task. Let the principles of treatment be mastered, and then quiet observation at the bedside will give one the experience which inspires confidence. A timely attention to the therapeutics of cardiac symptoms generally makes the use of heavy artillery unnecessary; but if we are obliged to bring this into the field, it should be borne in mind that it is unnecessary to place any limit to the dose of stimulants; if the weaker stimulants fail we must use the stronger, and increase the dose. In such cases the only limit is consistency; whoever is timid when life is at stake really belongs elsewhere than at the bedside."

I said that stimulants were indicated in nearly all cases. All practitioners meet some exceedingly light cases of pneumonia in which, it is true, we might get on without them; but I have observed that even these light manifestations of the disease progressed more favorably when stimulants formed an essential part of the treatment. One great point to be remembered is, that stimulants are to be given often. Every two to four hours I do not consider often enough. A dose of alcohol will wear off entirely in two or three hours, but by giving it hourly, or even much oftener, we can keep up and fully obtain the good effects. I have never employed camphor largely, and can therefore add nothing as to its usefulness. Yet Juergensen and several other great German observers are loud in its laudations. Just here let me quote the words of Robert Bentley Todd, since they bear directly on this point, and are the observations of one of the greatest observers

who has enriched the literature on pneumonia. He says: "I would say that in all cases pneumonia has, independently of this or that mode of treatment, a decided tendency to depress the general powers of life—in some more, in some less." In this observation all who have observed the disease studiously will concur, and I think also in the deduction I propose to make, that stimulants are required in this and all diseases in which the vital forces are likely to be overwhelmed. The carbonate of ammonia has been advocated as a remedy, exerting in some way a curative influence on this disease, and at the same time serving us as an excellent cardiac stimulant. Relatively to its claim as a remedy, having in some unexplained way a curative influence on croupous pneumonia, I take it there are no advocates to-day; yet, as a cardiac stimulant, expectorant, and diaphoretic, it is a remedy looked upon by many as applicable in a large number of cases. Attentive study in one's own practice of the results of treatment of this disease with the carbonate of ammonia, I do not believe will recommend great reliance upon the drug. Reports from time to time in medical journals of a large number of brilliant cures by different observers, should not wed us to a treatment if, under our observation, the treatment was barren of like results. It is a fact that carbonate of ammonia is inferior to alcohol as a cardiac stimulant—that its stimulating powers do not last long, and that it produces irritation of the stomach after prolonged use or its administration in large doses. This, as Prof. Loonis says, would cause an interference with nutrition, and "in that way diminish the chances of recovery." For these reasons I have not for some time used carbonate of ammonia in this disease.

In all the forms of this disease it has become a custom with me to give quinine at intervals throughout the active period of the disease. I generally order the sulphate in doses from three to five grains every four hours. This has appeared to exert a curative influence on the disease, which I suppose to be accounted for on the ground of its tonic action. While quinine is antipyretic in larger doses than this, it may be that even

small doses like this prevent a high range of temperature. On the use of quinine, Flint says: "Of tonic remedies, quinine is to be preferred. It is not indicated in mild cases, but whenever there are grounds for anticipating undue depression of the powers of life, it may be given and continued during the progress of the disease." When we suspect malaria as a complication, its use is imperative, and many good observers think its use in all cases, whether we suspect malarial complication or not, is good practice, since that poison often gives little evidence of its presence until its explosion in some of its characteristic icterus.

△ matter of very great importance if to give due attention to diet. A sufficiency of nutritious and digestible food should be administered, and no neglect of this matter must be overlooked. Milk, soft-boiled eggs, and animal broths, and some nutritive beef preparation, are useful articles of diet. Milk, combined with lime-water, is very useful when we are treating children, or when our patient is delicate. Prof. Loonis advises the administration wine along with the food, not as a stimulant, but to increase the digestion. My experience is that wine, given after food-taking, is of value, in that it assists the weakened digestive powers.

The question—shall we use antipyretics in the treatment of croupous pneumonia?—is an important one. A correct answer of this question can not be made until it is first settled to what extent fever is an element of danger, and whether the antipyretic at our disposal is free from danger. Many observers claim that fever, if it does not rise above 103°, is a conservative process. Also, that in cases of typhoid fever, in which the temperature was normal or subnormal, there were present all those symptoms we are in the habit of according to high temperature. Dr. Joseph T. Smith, in the course of an article on this disease, in the *New York Medical Record*, when speaking of antipyretics, says: "Shall the elevation of temperature be controlled—when and how?" This opens up such a broad field, and one so filled with combatants—it opens up that vast subject, the treatment of fever, which in itself would demand a

whole article to give even an outline view—that we may be pardoned if we but glance at the question.

We indorse the opinions of Dr. Whitaker, as expressed by him when discussing the subject at the meeting of the American Medical Association, 1888: "The bacteria," he says, "are sensitive to heat; 170° F. attenuates the bacteria of Frankel in from twenty-four to forty-eight hours." This corresponds pretty well with the clinical history of a case of pneumonia. The fever may be the very agent destroying the disease. It has been observed that those were the most favorable cases in which the temperature was most pronounced. Danger is not on the part of the fever, but on the part of the heart, and the question is whether it is not the best plan of treatment to sustain the heart, and let the fever alone; be content with small doses of antipyrine or antifebrin to do away with any evil effects of high temperature. Dr. Shattuck, at the same time, said: "During the past the influence of temperature in pneumonia has been overrated." He has discarded the systematic use of antipyretics in this disease. This, then, if an essential fever, should be treated as such, so far as high temperature is concerned. So long as it does not exceed 103° F. no harm is being done, and as a rule antipyretics should be withheld. It is known that antipyrine and several other antipyretic remedies exert a depressing influence on the heart, and this organ, by their use, is more heavily laden, and therefore more likely to fail. Yet, when the temperature is above 104°, and reaches 106°, it may be justifiable to give antifibrin. I should not, however, recommend such a course. If an antipyretic should seem imperative, I should depend first on sponging the body in tepid water. All of these agents are to be given with caution, and watched. It has been the longest time since I used antipyretics in pneumonia. I feel that I will not soon use such drugs in the treatment of the disease.

It must be our concern that all measures, of whatever nature, calculated to depress the heart, are rendered imperative. Failure of the heart is the most dreaded of complications, and allowing the patient to sit up when he is too

weak, must be one of the things that we must caution our nurse to observe. Digitalis is by some regarded as an essential remedy in pneumonia. Some writers have advocated the use of no other agent in the treatment of the disease. I believe if close watch be kept on the heart, and alcoholic stimulants given properly, we will find no demand for digitalis. Flint recommends it when the heart becomes feeble and irregular. This is an indication for its use, but I believe it useless to resort to it as long as the alcohol will afford the same results, and it is my experience that it will never be needed when alcoholics are properly given. It is generally advisable to administer throughout the course of this disease, a diaphoretic and expectorant; especially is this necessary if the skin is dry and harsh, or if there is much bronchitis present. A mixture containing syr. tolu and syr. ipecac, serves as well in this connection, and I frequently administer it.

When the nervous symptoms are out of proportion to the other elements of the disease, good results may be had from musk. This remedy was insisted on in the lectures on pneumonia by Frossau, and will be found valuable. Often the condition of the bowels will be a troublesome element in the disease. Where there is a decided biliousness, much relief can usually be obtained from the use of a saline cathartic. There are other minor points of treatment which will come up for consideration, impossible to outline in the limits of this paper, and which the nature of the particular case must afford the judgment of the practitioner the proper mode of action. We shall always be in the right line of defence, and always do the proper thing if we bear in mind Jeurgensen's words: "Nature cures, and the only duty of the physician is to maintain life until this cure is effected."

"I tell you," said the pert young assistant, "the editor isn't in, and I'm not going to tell you again. If you have anything for him you can leave it with me." "Very well," said the caller, taking off his coat. "I came in to give him a good, sound thrashing, but I'll give it to you instead."—*Harper's Bazar*.

Conan Doyle's First Novel.

S. S. McClure, in a recent interview, tells of his first acquaintance with Dr. A. Conan Doyle.

Mr. McClure said he was visiting Andrew Lang at St. Andrews University, in Scotland. Mr. Lang said: "There is a young man named Dr. Doyle who has written a capital shilling shocker (which is British for dime novel), and who is about to have a novel published by Longmans, and this man has a future."

"On my way back to Edinburgh," said Mr. McClure, "I purchased a copy of the shilling shocker, which proved to be one of the earliest and greatest of the famous Sherlock Holmes stories. I read it with unbounded delight. It was more than a shilling shocker. It was a great story, and, although a detective story, it certainly was a piece of literature."

"I continued my journey to London and purchased on the train copies of the two great critical journals, the *Athenæum* and the *Academy*. It happened that both contained a review of Conan Doyle's new novel, 'Micah Clarke.' Dr. Doyle's name was then unknown to literature, and the book was heralded as a great work by a new writer. The *Athenæum* scored the novel in strong terms, and expressed wonder that any publisher should dare publish a book so dull. The *Academy*, on the other hand, praised the novel, and rated it between 'Kidnapped,' by Stevenson, and 'Westward Ho!' by Kingsley. It is needless to say that the *Academy's* verdict was the right one, and the verdict of the public. I bought and read 'Micah Clarke,' and I instantly recognized the promise of this writer and arranged for his new novels and stories."

PAPA.—"Wasn't that a pretty rough game you were playing this morning? What was it, anyway?"

BOBBY.—"We were playing war. I was George Washington, and Tommy Jones was the American army."

PAPA.—"And who was that poor boy who seemed to be getting the worst of it?"

BOBBY.—"Oh, that was the new boy who has just moved next door. He was the British army."—*Harper's Bazar*.

VARIOUS USES OF PHENACETINE.

CLARENCE S. ELDREDGE,* M.D., PHILADELPHIA, PA.

In looking around for an antipyretic, we have these ideas in view: first, to select a drug that will reduce bodily temperature; second, that will be easy of administration; and third, that will have no deleterious action upon the system. A drug possessing these properties is phenacetine-bayer, which was prepared by Dr. Hunsberg and recommended to the medical profession by Kast in 1887.

Of late years I have used phenacetine-bayer, to reduce temperature in typhoid fever. I was called to see Mr. G. M., aged twenty-two, and found him suffering with violent headache and a sense of mental weakness; when standing erect his limbs trembled and he was seized with vertigo. He had no appetite, a bad taste in the mouth and was troubled with excessive thirst. The tongue was large, pale, swollen and indented at the margins by the teeth. There was abdominal tenderness and gurgling in the right iliac fossa, with aching in the back and limbs. Temperature was 104°; pulse rate 130. I put patient to bed and ordered a liquid diet, consisting chiefly of milk and whiskey, and prescribed phenacetine-bayer to reduce the temperature. I gave an eight-grain powder in the afternoon and the next morning found his temperature 101°, with a corresponding fall in pulse rate. I continued the phenacetine, giving eight grains every afternoon for twenty-one days, at which time the fever abated and the patient made a speedy recovery. This case never had any complications, the stomach retained food, the bowels moved regularly, and at no time did the temperature rise above 103°. In this case I can safely say that phenacetine relieved the aching of the first stage, reduced the temperature, and promoted an aseptic state of the alimentary canal.

Another case similar to the above might prove worthy of mention. Stella

M., aged five years, presented all the symptoms of typhoid fever. Temperature in axilla was 104.2; pulse rate 140. I gave, as before, milk and whiskey, and phenacetine-bayer in four-grain doses, to reduce fever, every afternoon. This case ran a course of fourteen days and made a good recovery. The patient had nose bleed once, but this was very slight.

In both of these cases after the first week I administered turpentine for a few days. It will be noted that in neither of them did I give any heart tonic, except whiskey as a food, and at no time did I see any unpleasant action upon the heart from the use of phenacetine. For restlessness during typhoid fever this remedy administered in the evening has sufficient anodyne properties to give your patient a good night's rest. In cases of this disease where an intestinal antiseptic is needed, I am in favor of giving phenacetine and salol.

In scarlet fever, phenacetine-bayer and the cold bath are to be used in all cases at the onset of the attack. Given early it reduces temperature, prevents restlessness, and modifies to some extent the course of the disease.

In diphtheria the same can be said of phenacetine; the hyperpyrexia is controlled, the heart's action not disturbed and the restlessness relieved by small and repeated doses of this drug.

In malarial fever I have given phenacetine to reduce temperature with happy results. In one case, a girl four and one-half years old, with evening exacerbations of fever and violent headaches, I prescribed phenacetine, reducing the temperature and relieving the pain. In the morning the temperature was always normal, while in the evening it rose to 106° in the axilla. Phenacetine was given by mouth and quinine by the bowel, each about four hours before the expected rise of temperature. This treatment soon prevented the chills, fever and sweating, and the patient improved rapidly. In all such

*Assistant in Ophthalmological Department, Medico-Chirurgical Hospital, Philadelphia, Pa.

cases I always start up secretions by first giving calomel.

In the so-called *la grippe*, phenacetine-bayer should not be lost sight of. In these cases, which, as a rule, begin with head and backache, pain in the limbs, and high temperature, it is the drug to use. It can be given alone in powder form or in combination with other remedies. One of my favorite prescriptions in these cases is: Phenacetine, grains three; quinine sulphate, grains two, in capsule form, every three hours. Equally good results can be obtained by administering Pill Phenacetine and Quinine Compound, as prepared by Schieffelin & Co., New York. After using the above treatment I have seen the pains disappear and the temperature reduced in a very short time. In *la grippe* of the aged and infirm, phenacetine is the most valuable drug, being given in small and repeated doses.

In *la grippe* associated with rheumatism in both young and old, phenacetine, together with salophen, gives very encouraging results. Phenacetine, grains two; salophen, grains two, in pills or capsules, every three or four hours, will not disappoint you. In cases where the heart action is weak or failing, I always give along with phenacetine strychnine sulphate, 1-40 to 1-60 grain in each capsule. We have here in strychnine both a general and heart tonic which will always benefit the patient.

In the various forms of headaches, such as hemicrania, and in neuralgias, phenacetine for the relief of pain is unrivaled, being prompt in action and lasting in its effects. In the headaches from eye-strain and muscular insufficiency it works wonders, but a cure cannot be expected except by glasses and the correction of the unbalanced muscles. One of my formulæ for the relief of pain in these cases is phenacetine, grains three; monobromated camphor, grain one; caffeine citrate, grain one, in pill or capsule. With this formula I have had excellent results. In cephalalgia of nervous origin I frequently prescribe a pill made by Schieffelin & Co. It is called by them Pill Hemicranine, and is composed of phenacetine, grains three; caffeine, grain one; citric acid, grain one; and with this I have had satisfactory results.

In people with idiosyncrasies for certain drugs it is not necessary to give phenacetine in combination. Being so nearly tasteless it is easily administered in powder form, and the other remedy can be increased or diminished as required by the patient without interfering in any way with the phenacetine.

Phenacetine in these cases will rarely, if ever, disappoint you if properly and judiciously used. Some cases require large doses, others small and repeated doses, but in my experience the results have always been the same, that is, relief of pain.

In pneumonia, phenacetine is a safe and efficient remedy. In those cases beginning with chill, pains in all parts of body, high temperature and restlessness, it is indispensable. It has a double action: first, antipyretic; sweating is produced and the temperature reduced like magic; second, analgesic and anodyne; the pain is relieved and the patient is made comfortable in a comparatively short time, and when given in the evening a good night's rest is secured. Experience in these cases must be your guide. I have administered phenacetine in small, repeated doses, while at other times large doses have given better satisfaction. I will refer to one case that recently came under my notice. Mr. J. C., aged fifty-six, clerk, came home in the afternoon with chills, headache, and a sense of general weakness. I found the patient with rapid pulse, high temperature, and expectorating yellow mucus. His breathing was labored, and the presence of a cyanosed condition was noticeable at the lips. Percussion revealed dullness at the base of the right lung, and auscultation gave marked bronchial breathing. After administration of phenacetine, the temperature began to fall, pain disappeared, the cyanosis lessened, the patient was made comfortable, and sleep seemed refreshing. Aside from phenacetine, I had patient wrapped in a cotton jacket and gave the usual expectorant treatment. This patient received a ten-grain phenacetine powder every day for eleven days, when the fever subsided, and the patient made a speedy recovery.

I must also call attention to phenacetine in acute articular rheumatism. When administered in ten to fifteen grain doses, three or four times a day,

good results have been obtained. I am not in the habit of prescribing it alone, but along with other remedies, such as salicylic acid, in capsules. I prefer, however, to administer phenacetine in powder and salicylic acid in liquid form, both at the same time, regulating the dose of each according to the susceptibility of the patient and the severity of the attack. Under this treatment pain and swelling subside and the disease disappears quickly and with complete recovery. In rheumatism associated with heart complications, as we often find it, phenacetine to reduce the fever is perfectly safe. Here we can give it alone or with strychnine sulphate. I have never seen any depressing action on the heart from its use in these cases. For rheumatism it can be combined with salicin, salol, and with salophen.

A combination of phenacetine and salol is highly recommended in cases where we have gastric disturbance. The salol is decomposed in the intestine and acts as an antifermentative and antiseptic.

In gonorrhœal rheumatism, probably the most obstinate of all rheumatoid affections, for both patient and physician, phenacetine acts well when given with iodide of potassium of salicylate of soda. I have had good results in one case in which my treatment consisted of phenacetine-bayer, iodide of potassium, and salicylate of sodium. The phenacetine was given in powder, the other remedies in liquid form, with blisters to the ankle.

My patient made a rapid recovery and has never had any after-effects.

In the treatment of tuberculosis, where we have fever, cough, and expectoration, phenacetine will prove of service. If given early in the afternoon it prevents the evening exacerbations of fever always accompanying this disease, allays, to some extent, the hard, harsh cough, and often lessens the expectoration. In some cases small and repeated doses work well, while in others it is better to give larger doses at longer intervals.

In whooping-cough, phenacetine has proved very satisfactory in my practice. When given early at the onset of the attack it modifies the spasm and diminishes the frequency and severity of the

paroxysms. I think phenacetine has but little tendency to effect a cure in these cases, but under its use the disease seems to run a shorter course and is certainly less severe. The dose is to be regulated by the severity of the attack and the age of the patient; one or two grains, three or four times a day, will usually suffice. To reduce the temperature of measles, phenacetine can be relied upon. Very recently I have treated several cases of measles with high temperature and marked catarrhal symptoms. I gave phenacetine in small repeated doses, with the result that the temperature came down and restlessness disappeared. Other remedies were given for catarrhal trouble. All the patients improved and made a speedy recovery without any complications.

For local application phenacetine has been used by me but very little. I have employed it in the case of old ulcers, etc., with benefit. When dusted on the raw surfaces it produces healthy granulations and rapid healing. I used it as a dusting-powder in one case of syphilitic ulcer along with antisyphilitic treatment. The ulcer healed and the pain soon disappeared. When employed as a dusting-powder, phenacetine should be finely powdered.

The variety of uses to which this drug has been put is almost numberless. I have employed phenacetine for a long time, and have found it a safe and efficient remedy. Its peculiar action and its manner of reducing bodily temperature is not clearly understood; still I have no doubt but that it acts directly on the thermogenic centre, increasing heat dissipation or diminishing heat production, or both at the same time. Cerna says the reduction of temperature results chiefly from a decrease in heat production, with a slight increase in heat dissipation. I have never seen any bad effects from the use of phenacetine, or any depressant action upon the heart. In administering antipyretic drugs in large doses, toxic symptoms should be looked for. The most striking symptoms would be cyanosis, more marked on the face, lips and finger-tips, a sensation of exhaustion, desire for air or vertigo.

Therefore, in all cases of weak and failing heart, where antipyretics are in-

icated, as a safeguard give such heart-tonics as caffeine, strychnine, etc.

In summing up the uses of phenacetine, I would say: First, it reduces fever and therefore is an antipyretic. Second, it relieves pain, hence has anodyne and analgesic properties. Third, it has a sedative action upon the nervous sys-

tem. Fourth, it has the advantage over other antipyretics of being non-toxic.

As phenacetine is almost tasteless and easy of administration, it can be given in combination with other remedies, or alone in powder, pill or capsule form.

COMMUNICATIONS.

NON-MALIGNANT STRICTURES OF THE RECTUM.*

JOSEPH B. BACON, M. D., CHICAGO.

The history of surgery shows that the treatment of non-malignant strictures of the rectum has been far from satisfactory and that the malady has baffled the skill of surgeons. While the patients were temporarily relieved, the inevitable return of the stricture and final obstruction of the bowel, or perforation of the ulcerated gut above the stricture, or the gradual amyloid visceral changes due to the pus infection from the ulcerated surface, gave results so sad that it was of small consequence to the patient whether the stricture was malignant or non-malignant.

The common practice among surgeons has been to gradually dilate or divulse, or to perform internal or complete proctotomy. Gradual dilatation by means of hard or soft bougies is an excellent palliative remedy for strictures situated in the lower third of the rectum, but in strictures above this point nothing can be more dangerous. It is the rule in stricture of the rectum for the mucous membrane, and often the muscular coats of the gut, to be the seat of ulceration that may vary in degree even to all but perforation into the perirectal space. Passing a bougie is very liable in such cases to perforate the thinned gut wall and so to cause death. At best, the treatment by bougies is rarely curative, and usually necessitates an indefinite

prolongation of the treatment and its accompanying risks. The accidents and deaths accompanying divulsion have been so common as to cause, practically, all surgeons to abandon this operation.

Linear internal or complete proctotomy has, as a rule, been ultimately disappointing, not only on account of the return of the stricture, but also by reason of the frequent occurrence of fecal incontinence after the division of the sphincter ani muscles. The incision, which leaves an open wound, gradually unites by granulation, and, with few exceptions, adds new fibrous tissue to the old stricture, and in from one to two years the patient's condition is as bad as, or worse, than before the operation.

Electrolysis has of recent years had its enthusiastic admirers, but the surgeons who have tried it are gradually becoming discouraged with it and at present consider the method but little if any better than gradual dilatation by means of bougies. When the period of aseptic surgery arrived, surgeons felt sure that by excising the stricture, even in those cases where the peritoneal cavity would necessarily be opened, and by reuniting the gut by end-to-end approximation, they would be able to successfully treat these strictures. The fatalities following the operation, however, made their statistics most discouraging. The tension upon the sutures was so great that frequently they gave

* Read before the Chicago Gynecological Society, December, 1895.

way, and infection of the peritoneal cavity resulted. In many cases the ulceration above the stricture was so extensive that suturing was almost impossible.

With the advent of the large Murphy button the results of end-to-end approximations were much better and the danger from stitch fistulas and peritoneal infection was reduced to the minimum. Thus the immediate results of the excision operation were a pronounced success. The remote or secondary results, however, proved a failure with every form of end-to-end approximation. The circular cicatrix left either by the button or the sutures acted as a center of irritation to the enormous amount of fibrous tissue in the rectal walls, and, together with the fascia in the pelvis, added new cicatricial tissue, and the gradual contraction eventually formed another stricture. It was exceptional for any case to terminate otherwise. The rectum is imbedded in fibrous tissue, and any wound of this organ must leave a more dense cicatrix than at other parts of the intestinal tract. It is a well-known pathological fact that the fibrous tissue adjacent to a cicatrix becomes hypertrophied and finally contracts, and thus adds to the original scar. If this be circular, as in the rectum, a mild stricture is formed, which, by irritation from peristalsis and the passage of fecal matter, becomes more and more formidable by the changes in the anastomotic fascias and connective tissue.

With the discouraging results shown by the literature upon the subject and with quite a number of unsuccessfully treated cases of rectal stricture in my practice, I determined to try to devise a method for permanent cure. I therefore began a series of experiments upon dogs and have devised a method that I think will permanently relieve all strictures situated above the levator ani muscles, and stricture in the female which extends down almost to the internal sphincter muscle, as the vagina can be separated from the rectum. The operation is practically as free from risk as an ordinary laparotomy, as the following case will show:

Dr. Effie Lobdell, of the Harvey Hospital, who sent the case to me, had the

patient under her care for the past year, had given her a long and thorough treatment with potassium iodide, and had frequently dilated the stricture with bougies. The doctor furnished the following history:

"Mrs. —; English; aged fifty-three; twice married; had twelve children by first husband, all well-developed and healthy except the last, which died soon after birth; also had one still-born child and two miscarriages. Patient has usually had good health. Mother died of old age, father of supposed cancer of rectum at fifty-five. Brothers and sisters all living and in good health.

"Patient has had gonorrhoea twice, and twenty-eight years ago contracted syphilis. A servant 'cured' her of the former. The husband administered mercurials to her for the latter, and she was salivated, losing all her teeth and hair. Had a general eczema over the trunk of the body. A physician finally effected a cure. During this year she was delivered of her last child, which showed characteristic marks of syphilis and died in a few days. Since that time her health has been excellent until three years ago. She has suffered from chronic constipation and hemorrhoids, both internal and external, for which she underwent operation at a hospital in 1892, during which the external sphincter was accidentally severed. Last summer she had an attack of nervous prostration; was in the hospital five weeks; the trouble in the rectum also began at this time, for which she received no treatment.

"She consulted me for vesical and uterine difficulty, which yielded to treatment, and I incidentally discovered the rectal trouble while treating her. She had had incontinence of feces for about five years.

"Upon vaginal examination I found the uterus anteverted, cystocele, laceration of perineum and cervix, prolapsed vaginal walls, and incomplete procidentia. The urethra was inflamed and sensitive. She gave a history of cystitis, and complained of frequent and painful micturition.

"Following up the uterus posteriorly with the index finger, I outlined a tumor several inches long and probably

two to three inches broad lying along the rectal wall.

"I prescribed for the patient potassium iodide in increasing drop doses, and the accumulation of tissue perceptibly diminished."

Upon examination, I found a tubular stricture extending from a point just above where the levator ani muscle encircles the rectum up beyond the reach of my finger, and so contracted that my index finger was barely able to enter it. The rectum was filled with pus, blood, and a great deal of mucus, denoting that there was a large ulcerated surface above the stricture. The external sphincter was greatly relaxed, and the patient was suffering from incontinence of feces as a result. The muscle had probably been overstretched or divided during the previous operation. I ordered the patient to bed and put her on liquid diet for a few days preparatory to operation. Instructions were given to thoroughly empty the bowels by means of high enemata of a solution of two drachms of ox-gall to one pint of water, and to avoid cathartics, as violent peristalsis might rupture the ulcerated gut.

June 22, 1895, assisted by Drs. Ferguson, Lobdell, Waite, and Stremmel, I made an incision into the abdomen in the median line extending from the umbilicus to the pubes. Then, by placing the patient in the Trendelenburg position and crowding the small intestines back with large flat sponges, I could see and feel the stricture and judge of its length, which was between three and four inches. The sigmoid was secured and bent down over the stricture to a point deep into Douglas' pouch below the lower limit of the stricture and the length required noted. The sigmoid was now drawn up to the abdominal incision, and at the point noted one-half of the smallest-sized Murphy button was securely sutured in place, and this portion of the sigmoid wrapped in a sponge and left in care of an assistant. Another assistant now inserted the other half of the button by means of a button-holder through the anus, up the rectum to the lower limit of the stricture, and turned the staff of the button toward the anterior rectal wall. By firmly pressing it against the wall I could feel and see the instrument from above, and by

gently nicking the gut over this point the small staff of the button protruded through the gut wall and was sutured in place and firmly held by the assistant with the button-holder. I next scarified the peritoneum over the stricture and sigmoid, the surfaces to come in opposition from one-half of the button to the other. Now, taking the half of the button in the sigmoid, I again bent the sigmoid down over the stricture so as to have the scarified surfaces come in apposition, and united it with the half of the button below the stricture, and thus formed a lateral anastomosis of the sigmoid and the rectum below the stricture. My intention was to suture the approximated surfaces of the rectum and sigmoid together along the scarified surfaces, so as to avoid the possibility of a loop of small intestine getting caught between them and being clamped off when the operation was completed. But on placing the lower half of the button, notwithstanding the great care taken to make only a small opening just for the staff, I noticed that pus and blood from the rectum entered the abdomen at this point and thus necessitated a drainage-tube with gauze from the bottom of Douglas' pouch. This gauze or Mikulicz drain was now placed and packed so as to firmly hold the sigmoid and stricture in apposition until the scarified surfaces could unite and form a firm septum from the button to the upper limit of the stricture.

The abdomen was now closed with silkworm-gut sutures up to the drainage-tube and the wound dressed with iodoform gauze, was covered with sublimate cotton and a firm roller bandage. Recovery from the operation was uninterrupted.

The drainage-tube was removed on the second day and the balance of the gauze drain on the seventh day. There were liquid bowel movements daily. The button came away on the ninth day, when an ox-gall enema was given and a good liquid stool secured. These enemata were continued daily and a good nourishing liquid diet continued until the patient gained strength. A clamp was placed without anæsthesia on the forty-seventh day, which came away on the fiftieth day. The patient was now given a generous diet of good variety

and rapidly gained in strength, and after the sixtieth day was up and around the ward.

I find upon examination now, although only five months have elapsed since the operation, that the greater part of the fibrous tissue has been absorbed, and that there is a large free opening between the rectum and the sigmoid quite sufficient for the passage of solid formed feces. The ulceration above the stricture has greatly improved and will probably soon disappear.

I used the small Murphy button, because all that was required of it was to form a fistulous tract between the sigmoid and the rectum below the stricture so the clamp could be applied to the septum. In this case I did not apply the clamp so as to cut away the whole septum, because in so long a tubular stricture it is not necessary to clamp away so much tissue. If from any cause it should be thought best to clamp away more of the septum it can be easily done, as there is comparatively no pain in the use of the clamp, which can be applied without an anæsthetic. I had this clamp constructed so that one blade could be inserted at a time and then locked. The slots in the handles were left so as to fasten a rubber ring over the handles after the clamp is first placed on the septum and thus gradually clamp away the tissues the first twenty-four hours; then, as the handles are approximated by the elastic pressure of the ring, they can be finally clasped together and the septum will be served in a few days. To clasp the instrument the first day might produce too rapid sloughing and thus set up a gangrenous spot that would not be limited to the septum.

The object in folding the sigmoid upon the stricture is to have a normal piece of gut united to the ends of the stricture band that has been severed by the clamp, and thus to prevent their reunion and reformation of the stricture.

In this operation there cannot be a return of the stricture, and the large opening left for the passage of feces relieves the irritation of the old stricture tissue, which gradually becomes absorbed. The ulceration of the rectum above the stricture disappears. The sphincter ani muscles are left intact and

continence is assured. It is impossible to place the lower half of the button in the rectum without infection of the peritoneum and therefore a drainage-tube, with packing of iodoform gauze, should always be used to insure drainage and wall off the general peritoneal cavity and thus limit the infection. A stricture formed in the lower end of the rectum extends so near the internal sphincter it would be rarely possible to use the method as described in the above case.

I have now operated upon twelve cases, the first one fourteen months ago. The patient has gained about thirty pounds in weight and there is a large free opening where a very tight stricture was formerly located, and I have every reason to think her permanently cured. Even this case is too recent to be assured that the recovery will be permanent. The other eleven are doing well, and in no case has there been any serious complication or sepsis.

The operation for stricture in the lower end of the rectum is simple and quickly performed. It consists in making a mucous fistula around the stricture from a point below its border posteriorly in the median line between the stricture and the coccyx, terminating in the rectum as an inner opening above the upper border of the stricture. A heavy silk ligature is passed by means of a blunt-pointed needle similar to an aneurism needle and left in place for three months, when it is removed and a probe-pointed grooved director passed through the fistulous tract and the stricture severed with a Pacquelin cautery down to the director. The object is to have a mucous tract at the bottom of the wound that will prevent the ends of the severed stricture from becoming reunited, as is the case in the ordinary linear proctotomy. Theoretically this is possible, but the cases are too recent to prove that the stricture will not recur; still, they show better results than occur after ordinary proctotomy, and this operation does not interfere with the sphincter muscles, which is very important.

"Take away woman," shouted the orator, "and what would follow?"

"We would," said a man at the back of the audience, promptly.—*Tit-Bits*.

CURRENT LITERATURE CONDENSED.

Reimplantation of Teeth After Complete Traumatic Dislocation.¹

My little daughter, aged two years and nine months, fell headlong down the cellar stairs and struck the two upper middle incisors on the edge of the step, extracting them as completely as if by forceps. The alveolar process of the right tooth was fractured, and the gum lacerated the entire length of the root. We found both teeth lying uninjured on the cellar steps. They were placed in a normal saline solution of tepid temperature. On the arrival of an assistant with chloroform the child was sleeping quietly. Chloroform was administered without the child awakening, and the teeth were placed within their sockets and pressed into position. The gums were cleansed antiseptically and the teeth left in position without further dressing or application. The teeth had been out of the mouth fully one hour. Milk and soft food were administered, and the lacerated edges of the gums cleansed after eating. It is now over four weeks since the teeth were placed, and they are perfectly solid, in good position, and of normal color. The gums are normal in color and consistency, and the appearance of the mouth quite natural. I report this as a successful case of implantation of teeth that had been out of the mouth over an hour, and as another demonstration of chloroform anesthesia during natural sleep.

The Treatment of Fibroid Tumors of the Uterus.²

This disease is so common that it must be considered by the general practitioner as much as, if not more than, by the specialist in gynecology. Some comparatively recent views on its treatment are now antiquated.

The advice to treat symptoms and trust for cure to the menopause is based on the erroneous assumptions that there are few real dangers to be feared from a uterine fibroma; that it begins to atro-

phy at the menopause and its symptoms to disappear; that operation for the removal or cure of the fibroid is followed by enormous mortality.

The tumor may undergo cystic degeneration with rapid growth and crowd the patient out of existence. Tubal disease is a very common accompaniment of fibroids. The tumor itself may suppurate, or it may become gangrenous, or it may become sarcomatous, or cause epithelioma of the endometrium. Every fibroid tumor causes increased vascular pressure, and cardiac disease is a very common result, and serious renal disease from direct pressure on the ureters is very frequently discovered. Even when the menopause is reached there is no certainty of relief. I have been obliged on account of urgent pressure symptoms to remove a calcareous fibroid tumor of the uterus from a woman eighty-two years of age. The period of the menopause is often postponed for ten or twelve years. The danger of operation is now eliminated, for it is less than the combined dangers to which a woman suffering from fibroid tumor of the uterus is constantly exposed. In small fibroid tumors of slow growth in young women, where the growth is not intraligamentous, and does not produce pressure or much suffering, the expectant plan of treatment may be followed, particularly if the woman is anxious for children. In older women, however, or in women who have been married several years and have not conceived, this consideration will not enter. It should always be remembered that the symptoms will probably increase in severity until the menopause, and that the menopause will be delayed. In the case of an old woman in whom the tumor has ceased growing, and in whom there is no discomfort from size or from pressure, operation is not indicated.

The choice of operation depends on the character of the tumor. Oophorectomy is not followed by as good results in cases of oedematous fibroids and soft myoma as in the harder forms of fibroid tumor. Cystic fibroids are of rapid and indefinite growth and are not relieved

¹E. T. Pettyjohn, M. D., *Chicago Clinical Review*, January, 1896.

²Charles B. Penrose, M.D. Ph.D., *University Medical Magazine*, January, 1896.

by oöphorectomy. When a uterine fibroid develops upward into the abdomen, it produces no symptoms until it attains a large size, but when it develops laterally between the layers of the broad ligament or backward and downward below the bottom of Douglas' pouch, it produces serious pressure symptoms, even when of small size. Castration for fibroid tumor must be completely performed, all ovarian tissue, and all of the fallopian tubes to the uterine cornua being removed. There are some subjective symptoms that are not dependent on the size or the direction of growth of the tumor. These are backache, headache, and reflex nervous disturbance, and are due probably to involvement of the body of the uterus, or to resulting disease of the endometrium, and disappear at once and forever on removal of the tumor. Hysterectomy is advisable in the vast majority of cases of fibroid. In all cases in which there are urgent symptoms from pressure, or in which there are urgent subjective symptoms referable to the uterus; in all fibrocystic oedematous, and myomatous tumors; in all tumors of intraligamentous or sub-peritoneal growth; in all large tumors which have become decidedly abdominal; in all cases in which we cannot safely and surely remove all ovarian tissue and the whole of the fallopian tubes.

Rheumatism as a Cause of Appendicitis.³

In a certain number of cases after the appendix has been removed, the patients still suffer from the same symptoms as before, and apparently the dyscrasia is in the cellular tissue or in the peritoneum in the immediate neighborhood of the cæcum. Sutherland, in the *Lancet*, reviews the series of investigations in regard to the large amount of lymphoid or adenoid tissue in the appendix, and which has led Bland Sutton to denominate it the "abdominal tonsil." From this similarity to the tonsil we might expect it to be attacked by pathogenic germs and to show gouty and rheumatic manifestations. Robinson, in the *New York Medical Record*, states that he has seen more than one patient who, subsequent to attacks of appendicitis, showed

decided articular manifestations, and during many years of this localization the appendical region remained free from inflammation. The rapid appearance of suppuration does not militate against this view, since the same thing may occur in acute rheumatic or gouty quinsy and salpingitis. When suppuration is fully established is it time for immediate operation, but in cases where only a small quantity of pus has formed, absorption may take place.

Obstinate Royal Invalids.

"A king has the right to die, but not the right to be ill," said Louis XVIII. to his doctors, forbidding them at the same time to publish the truth about his condition. Alexander I., perhaps in imitation of the Bourbon he had helped to the throne, acted upon the same principle, though he did not embody it in a paradoxical epigram. For more than forth-eight hours—namely, during November 12 and 13, 1825—he obstinately refused to be bled, notwithstanding the urgent persuasion to that effect of his own physician, James Wellye, and of Stoptingen, the medical attendant of the empress.

On the 14th, toward evening, Wellye, finding all persuasion useless, plainly told the Czar that, having refused the aid of science till it was too late, he had no resource left but the aid of religion. "And I have an idea that that will prove a broken reed to you," said the blunt physician, a worthy predecessor of Zacharin. "I am afraid that religion will be of little use to the man whose obstinacy in refusing all medical aid is tantamount to suicide." Thirty hours later the eldest son of Paul I. had breathed his last.—*Illustrated London News*.

A little girl in a Pennsylvanian town, in saying her prayers the other night, was told to pray for her father and mother, who were both very ill, and for one of the servants, who had lost her husband. She faithfully did as she was bid, and then, impressed with the dreary condition of things, added on her own account: "And now, oh God, take good care of Yourself, for if anything should happen to You we should all go to pieces. Amen."—*N. Y. Times*.

³Editorial in *American Medico Surgical Bulletin*, February 29, 1896.

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Editorial Staff:

A. L. BENEDICT, M.D.

W. A. NEWMAN DORLAND, M.D.

SAMUEL M. WILSON, M.D.

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PHILADELPHIA, SATURDAY, MARCH 7, 1896.

EDITORIAL.

TO PLUNGE, OR NOT TO PLUNGE.

In a recent number of the *Medical News*, Dr. A. L. Benedict, an editor of the REPORTER, expressed the heretical view that the frequent immersion of typhoid patients in cold water, is fraught with danger.

We recall the sententious remark of one of America's shrewdest clinicians—"I distrust, on general principles, any therapeutic measure originating with the Germans." The justification of this slur can be found by any who will read current German literature, and note the almost exclusive attention paid to pathology and physiology, the relative indifference to the relief of suffering, and the

disposition to experiment with human beings.

In referring to the water used for bathing as cold, we do not overlook the fact that, according to the rules of Brandt, it is rather warm for drinking, and none too cold for washing the face and hands, for a short swim by a person in health, or even for sponging the skin of a patient, limb by limb. We realize that some use water which is decidedly warm, and with these latter we have no fault to find except that the physician who orders a typhoid patient to be lifted from the bed to the bath-tub and back again, five to eight times daily, is responsible

for a disturbance of rest and an additional strain upon a heart already liable to succumb to the poison of an infectious disease. We question whether the purification of the skin and the stimulation of cutaneous vessels is compensatory. But the bath commonly used by all who may properly be considered followers of Brandt, even in a general way, is essentially a cold one, and there is added to the mere mechanical disturbance of the patient, the shock of sudden immersion in water 25° to 35° F. colder than the skin. It is inaccurate to speak of such a bath as ice-cold. We may grant the theoretical need of cooling and cleansing the skin, but we must not overlook the fact that a perfectly healthy person, especially if overheated, suddenly lowered into water of this temperature would experience a shock. We believe that there must be considered not only that the physician has to deal with 100 or 150 pounds of matter which is too hot, but the fact, which so many are prone to forget, that this matter has a nervous system; and that it must not only be reduced to a proper temperature, but it must be kept alive.

Our antagonism to immersion is increased by the fact that its advocates are divided against themselves—some maintaining that the treatment is essentially antipyretic, others that it is most emphatically not antipyretic—and that a marked depression of temperature throws considerable doubt on the presence of typhoid. Thus, while the two factions are agreed as to practice, the argument against them must be on distinct lines of thought. Of those who hold that the shock and fatigue of repeated cold immersion—few who have ever seen a patient in and after a bath will deny that shock and fatigue do result, at least as temporary sequences—we ask:

Is the elevation of temperature the essence of typhoid?

Have other means of treatment which have been successful in reducing temperature, materially influenced the course of the disease?

Does the average temperature range necessarily indicate the severity of the case—that is, unless an excessively high temperature is reached?

Is the indication to reduce temperature any greater than to supply nutrition?

If you use the crudest method of meeting the former, without reference to the weak heart, vulnerable kidneys and susceptible nervous system, why not be consistent and feed the convalescent patient according to the demands of his appetite and the obvious advantage of increasing nutrition?

If the fact that a typhoid patient is too hot is sufficient cause for sousing him in cold water, why shall we not also relieve a starving man by a liberal allowance of food, and revive a man dying of thirst by unlimited amount of water internally?

"See! Papa," is the delighted cry of the little boy, playing with his father's watch, "I have fixed it so the hands go round and round just as fast as can be." "Look at the increased renal elimination," triumphantly shouts the immersionist, who uses the bath not to reduce temperature but to improve phagocytosis and nutrition. He overlooks the fact that his treatment checks perspiration, drives the blood from the skin to the already congested and poisoned viscera and leaves only the choice between diuresis, diarrhoea and death. There is no doubt that cold bathing does stimulate phagocytosis, and other vital processes of which our knowledge is still more indefinite, but only in healthy or simply depressed systems. Yet the evil effects of injudicious use of this and similar hygienic means in persons not quite resistant enough, is too well established

to require more than the briefest allusion. The immersionists plunge almost every case of typhoid that comes into their ward, including anemic girls, flabby women, debilitated men, for whom cold bathing would be prescribed with the greatest caution (not more than twice daily) if they sought advice for the general conditions of weakness uncomplicated by typhoid. Even if we grant that the existence of typhoid is not so important a contra-indication of cold bathing as many comparatively insignificant constitutional states, must we also admit that in patients having both the dyscrasia and the infectious disease, the latter has conferred an immunity, and that they are, so to speak, above par in resistance because of the infection and elevation of temperature?

But the statistics. We can not refuse to accept the literal truth of figures, but

we call attention to the fact that comparative statistics usually cite the general death rate from typhoid at from 20—30 per cent.; the latter figure being certainly an exaggeration. Moreover, the contrasted mortality tables deal fairly enough, except with fulminating cases. But we venture to predict that a similar treatment of statistics would enable us to demonstrate the superiority of any method, not absolutely murderous, since it is the existence of fulminating cases that swells the general death-rate.

If the readers of the *REPORTER* will note briefly their views on this subject, and will send the notes to this office, we shall be pleased to tabulate them. *Opinions* will be valuable, but they should, in all instances, be sharply discriminated from *actual experience*, for or against immersion.

IEWS AND INTERVIEWS.

Within years less than a score, the world knew Philadelphia as the city wherein dwelt a professional guild, the embodiment of progress, whose doings and sayings were conceded authority in medical science; wherein existed facilities for study and observation unequaled in this country; and where flourished great business enterprises supplying the various needs of the profession. To all intents and purposes Philadelphia's present claim for recognition as a medical center is based solely upon superior facilities for educating students in medicine. The policy, which for ten years past has been energetically and unremittingly pursued, has resulted in centralizing attention on the educational institutions until these have grown so enormously in power and influence as to practically efface other independent interests, and to induce a condition of catalepsy in the unattached profession. Many of the once famous business enterprises no longer court publicity through professional channels, but apparently have sought in retirement rest from the labors undergone during the active

period of their careers. Philadelphia has just cause for pride in its magnificent medical schools, but it also has sufficient reason to question whether or not the abnormally rapid growth of these institutions has resulted in damage as well as in improvement. There are indisputable evidences of pressure atrophy apparent in the body medical.

* * *

To such proportions has the power of the educational institutions grown that they do not refrain from meddling in the affairs of organizations with which they have no connection, however remote. It has come to pass that the schools will dictate, solicit, or suborn, as the case may be, the appointment of some attachee to a position of trust or honor, perchance for no conceivable purpose other than to secure an addition to the ranks of their dependents. This quasi segregation of all available opportunities for professional work permits an exercise of influence analogous to that power attending the dispensing of

patronage in partisan politics. Subordination to a connection of greater personal importance, or even a divided interest, inevitably will work to the detriment of the minor connection, which, generally speaking, is the independent hospital.

* * *

Experience has never yet justified by results, a large accumulation of hospital positions in the hands of any one individual. Under existing conditions, a practitioner's capacity for efficient hospital service is limited. No matter what may be his reputation, for any man to occupy positions which he cannot fill to the utmost, is merely to deprive others qualified of the opportunity to do better work, more nearly meet the objects of the institution, and promote the interests of medical science. The number of appointments held beyond one's capacity to honestly and efficiently administer, is no longer regarded by the general profession as an index of the squatter's scientific qualifications so much as it is a manifestation of the "pull" the collector may possess, and the inability to conceal the porcine elements inherent in his make-up. This may be one reason why the independent hospitals of this city make no better showing in their work, and is, moreover, the reason why professional men unattached to the schools will make no effort to secure such appointments, deeming it useless to contend against this influence. The extent of institutional sway is strongly indicated when, as happens not infrequently, the conferment of the highest elective honors in societies presumably representative of the profession, is determined, not so much by a candidate's personality or achievements, as by a connection with the institution whose turn it may be to sport the laurels of the alleged honor.

* * *

In enlarging to the utmost their boundaries and in gaining all possible strength, the medical colleges of Philadelphia deserve cordial commendation. By constant and well-directed efforts they have greatly increased their resources through large contributions and endowments from private individuals, and by enormous State appropriations. They have provided the best of modern facilities for giving instruction to undergraduate students; they have made no small provision for research work in special lines; they have raised the standard of medical education to the

highest practicable point; and they have attracted to their doors students in great numbers and from all parts of the civilized world. Better opportunities for securing a thorough medical education can be found nowhere in the Americas, and Philadelphia is justified of these her children. If, hereafter, with all the resources now at command, the teaching faculties of Philadelphia fail to earn glory even greater than that of the past, the lack will be fixed with tremendous emphasis on the personnel of the faculties themselves.

But all this rapid growth and increased power of institutions, has served only to prove that undergraduate schools, no matter how great and excellent, are, of themselves, in no wise adequate to supply the necessities of a modern medical center; and if such schools alone are to be magnified and other features left uncared for, Philadelphia may as well accept without protest inferior position in the ranks of American medical cities.

Not the Sign He Was Looking For.

A good-natured Irishman rushed into the office of a broker one day, wearing an anxious expression on his face, and asked: "Have you a closet handy, Oi d'now? Oim quite unaisy, sir!" He was directed out the back door, and told to turn to the right, where he would see the sign. Having occasion to step outside himself in the course of a few moments, the broker found his guest squatting alongside of the wall of the office. With an expression of anger the broker asked Pat why in thunder he didn't follow instructions and go to the closet. "Begorra," says Pat, "that sign says 'urinal,' and indade, sir, I had to have an arsenal!"

Robert Louis Stevenson told a Washington writer that his story of "Dr. Jekyll and Mr. Hyde" had for its foundation an incident related to him by a London doctor who made diseases of the brain a specialty. None of his work was absolute fiction, and most of it had a basis in actual experience. "I do not believe," he said, "that any man ever evolved a really good story from his inner consciousness unaided by some personal experience or incident of life."—*New York World*.

ABSTRACTS.

STRANGULATED INGUINAL HERNIA OF A CYSTIC
APPENDIX VERMIFORMIS.*

WELLER VAN HOOK, A.B., M.D., CHICAGO.

Leopold M—, German by birth, 43 years old, weight 170 lbs., height 5 feet 9 inches, newspaper dealer, had had good health (except an attack of typhoid fever at fourteen years of age) up to four years ago. At that time he had a feeling of soreness at the umbilical region, and a sudden movement or a concussion or pressure upon the abdomen would cause intense pain. As a result of over-exertion in lifting, a right oblique inguinal scrotal hernia was contracted one and a half years ago. The abdominal tenderness gradually increased up to the time I first saw him. He never suffered from indigestion or griping pains. Has always been constipated, the bowels sometimes not moving for a week. He never noticed a tumefaction in the abdomen, but on pressure over the iliac and hypogastric regions he felt a sensation of tenderness, but this soreness was not greater upon one side than upon the other. The hernia always disappeared without taxis on lying down, until September 16, 1895, when he noticed that the hernial contents remained in the sac. His own efforts at reduction being unsuccessful, Dr. J. W. Dal was called, after thirty-six hours. He found a tense swelling in the inguinal canal which he could not reduce without anaesthesia. The patient was vomiting fluids from the upper intestinal tract. No gas had passed by the rectum, but this fact may have been due to lack of sufficient peristaltic activity. Under chloroform Dr. Dal again attempted taxis, and the tumor seemed to disappear. When the patient recovered from the anaesthetic, however, pain was greater than before and took on a different character, radiating over the lower abdomen toward the umbilicus. The doctor's observation at the evening visit convinced him that

the patient's condition was not satisfactory, and a consultation was requested. On examining the patient I found him suffering abdominal pain, with a pulse of 90, and slight elevation of temperature. There was tenderness over the lower right side of the abdomen; in this region the muscles were abnormally rigid, and palpation was difficult. The abdomen was slightly distended. In the inguinal canal and extending into the scrotum was a brawny mass, not very tense. It seemed most reasonable to suppose, in the absence of clearly marked diagnostic signs, that reduction had been imperfectly accomplished, and that a thin mass of omentum was left in the sac.

After the usual preparations, the sac was exposed by a sufficient incision and carefully opened. To my surprise, no omentum or intestine was present. The brawny sensation was due to acute inflammation of an unusually thick sac. The peritoneal lining of the sac was injected and rough, and the walls edematous. In the sac, lying in scattered masses here and there, was a quantity of translucent amber-colored mucus, having almost exactly the appearance of calves'-foot jelly, broken up into pieces of one to ten millimeters thick. Its consistency, however, was somewhat tenacious. Having seen on another occasion the same material in a cyst of the vermiform appendix, I recognized at once that such a cyst had been strangulated in the inguinal canal and ruptured in reduction.

The incision was prolonged sufficiently to open the abdominal cavity, where an additional quantity of mucus was found scattered over the intestines and the parietal peritoneum of the internal iliac fossa, sufficient to make the whole amount equal to at least an ounce. The caput

* *Medicine*, March, 1896.

coli was near at hand, and on drawing it into the wound the cystic appendix was drawn into the wound. At the distal extremity of this appendix was found a rent as large as a lead pencil, through which mucus was protruding. The walls of the appendix were dilated at the middle, but near the cecum was a constriction. The caput coli and neighboring small intestines were covered with thick tenacious masses of false membrane, quantities of which were stripped off. There was but a small amount of fluid present. The peritoneum, where not covered with false membrane, was deeply injected and roughened. That these changes involved an extensive area of the serous membrane was proved by cautiously drawing out the neighboring coils of bowel.

The sac of the hernia was first dissected free and cut off at the celiotomy wound, a number of iodoform gauze capillary drains were applied carefully in different directions in the abdomen, and the caput coli drawn well up to the wound. The appendix was then ligated close to its base and amputated with scissors. The suture used to ligate the appendix was left long enough to be passed through the eye of a needle, and was then carried through the muscular walls of the abdomen. In this way the cecum, whose walls were more violently inflamed than those of other parts of the intestines, was anchored close to the abdominal wound. The celiotomy opening was, for the most part, left open to facilitate drainage, although a few silk-worm-gut stitches were inserted to be tightened as secondary sutures.

The patient bore the operation well, and despite the grave local infection made uninterrupted progress toward recovery. During the first few days an enormous serous discharge escaped from the opening, and the temperature went up to 101.2°. The pain disappeared, however, and, the bowels responding to laxatives, the general condition of the patient rapidly improved; the wound healed by granulation, and the man now, three months after the operation, attends to his usual business, wearing a truss to protect the abdominal walls.

The portion of the appendix removed and in the recent state is six centimeters in length and about three centimeters in

diameter. Its outer surface is of a bright-red color, except at points covered by a grayish-red false membrane. Here and there are small masses of an amber-colored, thick, tenacious substance which finds its way out of the appendix, on pressing its walls, through the cut end and through an irregular opening at the distal extremity about eight millimeters in diameter. The shape of the appendix is irregularly cylindrical; but about two centimeters from the distal extremity the tube is bent upon itself from the longitudinal axis in the direction of the mesenterium at an angle of about 40°. The walls of the appendix vary in thickness from about four millimeters at the point of amputation to nine or ten millimeters near the distal extremity. There are also variations in the thickness of the walls at the same level, giving rise to slight pouchings of the lining membrane at several points. But these pockets do not at any point reach the dignity of diverticula. The interior of the appendix is filled with the same gelatinous, amber-colored material already noted as having been seen adherent to the peritoneal surface of the process.

Microscopically, the fibrous coats of the appendix are greatly thickened. The muscular layers are not thicker than normal. Out of thirty sections examined, only one shows a small ill-developed layer of epithelial cell representing the atrophic mucous membrane, the place of which elsewhere is filled entirely by a layer of connective tissue.

This case is unique in the occurrence of rupture of the cystic appendix in the sac of a hernia; it is the third case of which I have knowledge, after a study of the accessible literature, where a cystic dilatation of the appendix occurred in a hernia, and the ninety-ninth case of hernia of the appendix. It is also interesting from the fact that inflammation of a hernial sac had its origin in a lesion of the appendix.

Rupture of the cystic appendix by taxis in the effort to reduce an inguinal hernia is an accident which, as has been already remarked, has not before been recorded. And even if such a possibility were known, no practical advantage of the knowledge could be taken, both on account of the rarity of the circumstance

and the want of diagnostic signs to lead to a proper knowledge of the conditions before operating. The practical lesson to be gained from an acquaintance with this case is that the contents of a hernia are practically unknowable before the sac is opened, and that taxis is dangerous in a certain proportion of cases. But little persuasion is needed to convince surgeons to-day that the open method is, in the great majority of instances, the simplest, most radical and satisfactory treatment for strangulated and incarcerated hernias.

Cystic enlargement of the vermiform appendix seems first to have been noted by Virchow, who had observed a case in which the appendix was distended to the size of a man's fist, and as the contents were a thin watery fluid he called the condition hydrops of the vermiform appendix.

Rokitansky, in his text-book of Pathological Anatomy, said: "We further occasionally observe a metamorphosis of the vermicular process produced by obstruction which is analogous to dropsy of the efferent ducts of glands, and which is most apparent in the gall-bladder." (*Hydrops cystidis felleæ*.)

Glasmacher's case was that of a soldier who presented himself with a tumor the size of a pigeon's egg in the right inguinal canal near the abdominal ring. For a year there was no trouble. Suddenly obstruction occurred, and on making a herniotomy a cyst holding a drachm of pus was found. This cyst contained no feces or gas. A pedicle extended into the abdominal cavity. The patient died with continued symptoms of obstruction. At the autopsy intestinal gangrene was found. The cyst was proved to have been the appendix. It does not seem to have been a true retention cyst, although it has been referred to as such by some authors.

Zdekauer's case was that of a woman, 18 years of age, who had a recent right inguinal incarcerated hernia, the contents of which were diagnosticated as an ovary. On exposing the mass it was found to be a vermiform appendix dilated to the size of an acorn and filled with pus. This case, like that of Glasmacher, I cannot regard as a retention cyst on the evidence of the recorded histories, although Bajardi classifies it in

that category. The condition was simply that of pus-retention within the imprisoned appendix.

Wölfler's case was operated upon by Billroth for hernia. No intestine was found; but a cyst, thought at first to be the sac, was opened, allowing a teacupful of serous fluid to escape. The herniated cyst was found to communicate with a larger cystic cavity within the abdomen extending from the psoas muscle to the small pelvis. Pressure on the abdomen caused a bloody fluid to escape. It was thought the symptoms of strangulation had been brought about by the tenseness of the cyst. In spite of free drainage the patient did not do well until the lining membrane of the cyst, after several days, had sloughed and been removed with forceps. The thickness of the cyst-wall was between three and seven millimeters.

Steiner describes four specimens in the Pathological Museum of Basle, giving chief attention to minute anatomy. *Specimen 1* was from the body of a woman, 30 years of age, who died of puerperal fever. The contents of the cyst were homogeneous gelatinous masses. No communication existed between the cyst and the cecum. The form of the cyst was elongated, biscuit-like, two dilated parts communicating by a narrower part. The entire length of the appendix was 5.5 centimeters. The thickness of the wall varied between 1 and 2½ millimeters. The inner surface of the cyst showed a shining, smooth, membrane with striated markings. No trace of epithelium or Lieberkühn'st glands. Even the muscularis mucosæ and the follicles had disappeared. The wall of the cyst consisted of three layers—an inner connective-tissue layer, and a layer of loose cellular tissue uniting the first layer with the well-developed muscular layer, consisting chiefly of circular fibres. *Specimen 2*: The appendix was 8.8 centimeters long; the cystic dilatation had a longitudinal extent of 5.8 centimeters with a transverse diameter of 1.8 centimeters. *Specimen 3* was also that of a small cyst; the mucous membrane was not wholly destroyed. *Specimen 4* occurred in the person of a 55-year-old woman. The cyst was small, and the muscularis mucosæ, together with some epithelium, was retained *in situ*.

Steiner states that in 2280 protocols of autopsies made in Basle, only three cysts were found. In the cases with moderate dilatation a distinct hypertrophy was found in the circular fibres of the muscular layer, while in the larger cysts with marked stretching of the wall the musculature was not thickened. Steiner, like other observers, noted the occurrence of diverticula in the walls of the cysts. His discussion of the microscopical relations of the walls of the cysts is interesting, but too long to be quoted.

Shoemaker published an account of a case in which a cystic dilatation of the appendix was discovered post-mortem, no symptoms having been noted during life.

The six post-mortem specimens of Ribbert had fortunately such varying characteristics that he was able to formulate the changes which take place in the appendix when cystic dilatation occurs. He says: "In the lesser degrees (of dilatation) we see no essential alteration in the composition of the wall. Later, abnormalities of the epithelium occur; it is lost, together with the glands, throughout considerable areas, and forms in other parts a covering of a single layer without glands, which, however may still be present in patches; in very great dilatation it is destroyed altogether, but, as Case VI shows, disappears even when dilatation is slight. We must, however, take into consideration that the loss of epithelium does not need to be the result of dilatation, but that it may be destroyed by the disease process which led to the cicatrization producing the obliteration of the lumen. The follicles disappear equally soon, and finally are altogether wanting." Ribbert adds that an obliteration interrupting the lumen of the tube does not necessarily lead to a cystic dilatation, although the glands may be retained. Bischoff thinks that a dilatation is wanting when the mucous membrane is still able to resorb in the normal way. Ribbert maintains that a dilatation is also wanting when, as a result of early and extensive destruction of the mucous membrane, no secretion into the lumen can occur.

Finkelstein describes a specimen in Sonnenburg's collection in which the ap-

pendix was dilated to a length of 14 centimeters and a circumference of 21 centimeters. The pear-shaped appendix appeared like a pedunculated tumor.

In the case reported by Wenzel-Gruber was a cyst six centimeters long, attached to the cecum by a pedicle four centimeters long. The cyst had a transverse diameter of $4\frac{1}{2}$ centimeters, and was filled with viscid mucus. This case was remarkable for the fact that the obliteration of the lumen was brought about by a chronic tubercular catarrh.

Kelynack relates a case of a middle-aged female who died from extensive vegetative endocarditis, in which the appendix was found to be completely shut off from the cecum, and no sign or indication of any previous communication could be observed. The appendix was greatly distended and presented two very distinct diverticular processes, which were directed between the folds of the mesentery of the appendix. The diverticula were connected with the dilated cavity of the appendix through well defined circular openings. The appendix contained a thick gelatinous light yellow substance, and also a small quantity of material having the appearance of curdled milk.

J. A. Berry refers to a case of Féré described as a *mococoele* of the appendix, and Berry himself describes a case in which (as in Shoemaker's case) the appendix was found post-mortem distended with thick gelatinous mucus.

Orth and Leube give descriptions corresponding to the older accounts of Virchow and Rokitansky.

Bierhoff gives a report of three cases of this condition.

Förster is cited by Wölfler as having had a case.

Kelynack, in addition to his own case, refers to reports by Fenwick and Coats. The latter states that the appendix had been converted into a large cyst, measuring five inches in its long diameter; the cyst contained a tenacious colored material, and the wall was thick and firm. In Fenwick's case "the appendix was distended by a milky fluid, the communication with the cecum being obliterated."

In February, 1895, I witnessed an operation by Mr. Frederick Treves at the London Hospital, in which the ap-

pendix was enlarged cylindrically, its length appearing to be about $4\frac{1}{2}$ inches and its diameter one inch. What symptoms had occurred to demand an operation were not stated. The appendix was amputated after the formation of cuff-like flaps, which were united over the stump. The cyst contained a thick gelatinous mucus. Mr. Treves stated that a specimen almost exactly similar to the one removed was deposited in the Cambridge Museum. Mr. Treves' case is the only one operated on for symptoms arising in a cyst not involved in a hernia. A record of the symptoms produced would be of great interest.

A most interesting case is that of Maylard, who found at a post-mortem upon a patient dying of Bright's disease a dilated appendix measuring four inches in length and two inches in thickness. The dilated part was filled with a clear gelatinous substance which could be turned out *en masse*. At the proximal end it communicated freely with the cecum. Perhaps we would be less surprised at the presence of so much mucus if we remember that it is not uncommon to find a viscid plug of mucus in the normal appendix. And as Maylard's case seems well authenticated, we must assume that absolute obstruction is not wholly necessary to the accumulation of thick mucus. It must be otherwise when the fluid is limpid, as in Virchow's and Rokitsky's hydropic form. As in my case the base of the appendix was ligated before being removed, no attempt was made to determine the permeability of the proximal part of the process, but the occurrence of active plastic peritonitis strongly indicates the partial patency of the tube, permitting the exit of micro-organisms from the cecum. The contents of such cysts containing thick mucus cannot, therefore, be regarded as sterile without further investigations. It would seem probable that micro-organisms were absent from the cysts whose contents are limpid. In the case of the cysts whose contents are viscid and gelatinous, the stiff and unyielding character of the material accounts for its failure to be expelled through the contracted opening, while the more fluid secretion of the mucus could easily escape.

The total number of cases observed

is, therefore, so far as I can discover, thirty-two, including the cases of Treves, Hektoen, and myself. This number indicates the rarity of the disease. Yet I cannot help thinking that the innocent character of the lesion has prevented many observers from reporting cases.

The symptoms produced by cysts of the appendix are very slight, if present at all. In my case there were tenderness and sharp pain on jarring the body, for four years prior to the strangulation of the hernia. But the significance of these symptoms cannot as yet be determined.

In the absence of characteristic determinative signs the lesion cannot be diagnosticated. It will be as much as we can do, as Senn says, to bear this condition in mind when we have to deal with cysts in the region of the appendix. And my own case would cause us to bear in mind the possibility of cystic dilatation when the patient is long afflicted with vague tenderness, aroused into sharp pain by concussion or pressure.

Hernia of the vermiform appendix, uncomplicated by the presence of other visceral parts, is an unusual occurrence. Klein, Brieger, Bajardi and others have collected cases to the number of ninety-eight, to which we may add my own.

Hernias of the vermiform appendix are internal (with which we are not at present concerned) and external. Of the latter, Bajardi found fifty-seven inguinal, forty crural, and one obturator. Considering the relative infrequency of crural hernias, the proportion in this collection seems so great that special causes would seem to operate in its favor. Two circumstances, it seems to me, may be adduced in explanation. First, the small femoral opening might admit an appendix when a coil of intestine might not enter. And secondly, in inguinal hernias, the greater distensibility of the rings would as a rule permit the entrance of other parts of the gastro-intestinal tube.

The clinical phenomena of strangulated appendiceal hernia are very variable, and have been analyzed by Bajardi for the ninety-eight cases he collected.

The symptoms of anti-peristalsis were present in many cases, continuous vomiting occurring fourteen times; nausea or

efforts at vomiting occurred in five cases. Constipation was absolute in sixteen cases, incomplete in five, and wanting in eight.

The causes of the phenomena of incarceration are difficult to learn. In Glasmacher's case an acute flexion caused the obstruction, and actual obstruction doubtless occurs sometimes. A few cases may be explained by Klein's supposition that the obstruction is a dynamic one, due to the reflex involvement of the motor nerves of the intestinal musculature.

Brieger refers in this connection to the experiments of Kirstein, who found that ileus is not brought about by the mere sudden occlusion of the intestinal lumen, since the dogs in which intestinal occlusion was brought about by suture died after a rather long time from hunger, while the stormy symptoms which arise soon after the occurrence of incarceration develop as a result of the maltreatment of the strangulated intestine and its nerves. The violent symptoms caused by the strangulation of omental hernias are also to be borne in mind.

The mechanical conditions upon which hernias of the appendix depend are an abnormally situated cecum, an unusually long meso-cecum, or an excessively lengthy appendix. In the recorded cases the appendix is not said to have been extremely long. It would seem that usually the appendix has been able to reach the hernial canal by a post-natal slipping-down process on the part of the cecum, which Hildebrand, who has studied eighty cases of hernia of the cecum, thinks is the rule in such cases. But the occurrence of twenty-one congenital hernias among Bajardi's ninety-eight would lend color to the explanation of Brieger, who states that in fetal life the vermiform process maintains a band-like connection with the testis, or is immediately adherent to it, so that when the testis descends the appendix is drawn with it. The natural query is: Why does not congenital hernia of the appendix occur oftener, if this is correct?

Another etiological fact suggested by Brieger is the patency of the processus vaginalis. Of course the appendix could easily descend into it upon the occurrence of exciting causes.

Inflammation of a herniated appendix is prone to occur, as evidenced by Bajardi's thirty cases. The peculiar situation doubtless favors the occurrence of infection, but the presence of foreign bodies in eleven cases accounts for the phenomenon in these instances. Perforation took place in sixteen cases, nine times at the end of the appendix. The appendix was gangrenous in four instances. Diffuse peritonitis occurred seven times as a result of perforation.

The symptoms of hernial appendicitis are those of acute inflammation of greater or less violence in a hernial sac. And if strangulation is added, the symptoms of ileus still further confuse the picture. The diagnosis is very difficult in both strangulation and inflammation of herniated appendices. In forty-seven cases of strangulated hernias of the appendix, the diagnosis was, according to Bajardi, only twice made before operation. In Löbker's case Hueter made the diagnosis of probable incarcerated hernia of the appendix on account of the existence of phenomena very similar to those observed in two other cases which he had already seen. In Jackle's case Roser had admitted the possibility of hernia of the appendix on account of absence of vomiting in the presence of intense local symptoms.

Bajardi's comment that diagnosis of those conditions will be almost always impossible, would seem to be justified when we remember that the symptoms are often so violent as to simulate closely a strangulated hernia of the small intestine. In one case a diagnosis of inflamed appendicular hernia was reported by Jackle. A child which had been cured in the clinic of a suppurating inguinal hernia returned, presenting a solid cord within the scrotum. The accurate observations previously made rendered the diagnosis easier.

The mistakes made in diagnosis have been varied and sometimes very misleading. The inflamed mass has been at different times considered a peri-orchitis, an orchitis, and an inguinal adenitis; while in one instance the surgeon was in doubt as to the existence of proctitis or coxitis. In the case of Gibney the symptoms closely simulated those of a coxitis.